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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The use of tunnel concentration profile data to determine the ratio of NO_2/NO_x directly emitted from vehicles" *by* X. Yao et al.

Anonymous Referee #2

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This paper reports measurements of longitudinal NO, NO2 and O3 profiles in two tunnels of c.4 km in length in Hong Kong from which the authors conclude that the primary on-road vehicular NO2/NOx ratio was less than 2%, substantially lower than is usually quoted for this ratio. Reliable information on this primary ratio is important for accurate modelling of ambient NO2, which is subject to air quality standards in most countries. The general aim of the work undertaken is thus highly relevant and within the scope of ACP.

However, the amount of data and detail provided in the paper are both relatively

slight and consequently I have reservations that there is sufficient substantiation of (or caveats to) the main conclusion to justify publication at this stage. The authors attribute observed concentrations differences between the north and south-bound tubes of the Tai Lam Tunnel to different fuel characteristics which shows that fuel/fleet characteristics are important in interpreting and reporting data.

I have the following additional points:

(1) The authors cite literature from 1979 (Hilliard and Wheeler) and 1983 (Lenner and Lindqvist) for previous and comparator presentations of vehicle primary NO2/NOx ratio. Are such comparatively old measurements relevant to the modern vehicle fleet given the huge changes in engine and exhaust technology in the lost 25 years?

(2) To what extent are measurements from the mobile platform genuinely representative of the "ambient" NO, NO2 and O3 concentrations at the point in the tunnel at which the measurement is taken, as opposed to being influenced by emissions from the vehicle in front?

(3) The stated ventilation rates for both tunnels are large which I crudely estimate might give rise to several air exchanges per hour. Thus the effect of external air introduced into the tunnel may be greater than the authors acknowledge. The authors also provide an estimate for an in-tunnel windspeed of ˜5 m s-1 (p12729) but make no comment on the direction of this wind flow: is the wind flow in the tunnel moving with or against traffic flow? is it moving outwards in both directions from the centre of the tunnel?

(4) The authors should investigate in more detail a total OX (NO2 + O3) approach of evaluating their data as described by Clapp and Jenkin, Atmos. Environ. (2001).

(5) The authors discount the termolecular reaction of NO+NO+O2, yet the NO concentrations presented are likely high enough for this reaction to contribute to generation of NO2.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 12723, 2005.